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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tracking the emission and transport of pollution from wildfires using the IASI CO retrievals: analysis of the summer 2007 Greek fires" *by* S. Turquety et al.

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First of all, the authors wish to thank the two referees for their helpful comments and suggestions which will greatly improve our paper. In particular, the revised version of the manuscript will follow the suggestion to rewrite the introduction giving less general information but more specific descriptions relevant to this paper. We also agree with reviewer 2 that the conclusions on the vertical information can be viewed as too optimistic in the current version of the paper, since there is no clear evidence that plume height in the lower or middle troposphere can be differenciated. The abstract and conclusions will be modified accordingly in the revision. The revision will also include all technical corrections, including the modification of the figure legends.

We detail below a response to specific comments by both referees. Before each response, we have copied the reviewers' comments in italics. The corresponding information and/or corrections are added to the revised version of the manuscript for each point.

Anonymous referee 1:

1-3, 5, 9-11, minor comments: All suggested modifications have been done.

4 – p.7418 line 18:
Why is the correlation between CO and aerosols explored and not e.g. the correlation between CO and O3? Is there a special reason for this? What does the correlation between CO and aerosols tell you about the fires?

The analysis of the correlations between CO and aerosols is important here because we want to analyze the vertical information contained in the IASI observations. The only available vertical profile within the transported plume is provided by the CALIPSO lidar soundings. A correlation of the CO and AOD allows a preliminary comparison of the transport pathways of trace gases and aerosols.

It is also a first step towards an analysis of the interactions between aerosols and photochemistry. This part is beyond the scope of this paper. Correlations between CO, O3 and aerosols in these fires plumes are currently being analyzed using a regional chemistry-transport model to evaluate the ozone production in the plumes, and thus their impact of regional air quality. This point will be clarified at the end of the introduction and in the conclusion.

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6 – *p.7421 line 3: How many iterations are typically needed to reach convergence?* Less than seven iterations are typically required to reach convergence.

7 – *p.7421 line 10: How do the measurement error and model parameter error compare? Are these of the same magnitude or is one significantly larger than the other?*
The measurement error and the error due to the temperature profile are typically of the same order to magnitude and twice lower than the smoothing error (Barret et al., 2005). This is a rather coarse approximation since the uncertainty on the temperature profile from ECMWF is not known for fire plumes. It could be larger for the fire plume.

8 – *p.7422 line 22: Do you use the temperature and water vapor profiles as well as the surface temperature from ECMWF data? What surface pressure do you use? The ECMWF surface pressure? What is the temporal and spatial resolution of the ECMWF data used? E.g., 1x1 degree or 0.5x0.5 degree; 3 or 6 hourly data? Have the ECWMF data been interpolated to the overpass time and foot print of IASI? It would be worthwhile to add this information to the text.*
The surface temperature is retrieved from the IASI spectra using the atmospheric windows, only the temperature and water vapour profiles from ECMWF are used. These are from the operational analysis, i.e. 6 hourly data, at horizontal resolution 1.125 x 1.125 degree, and on 21 levels. The analyses are then interpolated to the IASI measurement location and time.

12 – *p.7424 line 20: Do you mean 'outside the plume regions' ?* Yes, of course, this refers to 'background' CO values, outside the fire plumes. We now state this clearly.

13 – *p.7425 lines 4-10: What is the total uncertainty of these measured mixing ratios?*

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Interactive Comment

The value of 22 ppmv close to the fires is still a factor of 5.5 larger than the value reported by Hobbs et al. close to the savanna fires. Do you expect such a large difference based solely on the different types of vegetation burned? The value of 22 ppmv is dominated by the value found for the level closest to the surface (see Fig.4). If the lack of sensitivity near the surface in this case is caused by saturation of the absorption lines then the retrieved values are expected to be much more sensitive to calibration errors than for unsaturated absorption lines. Hence, I would expect a large uncertainty in the retrieved value and possibly an overestimation of the value rather than an underestimation as stated in line 22. Can you elaborate a bit more on the effect of saturation on the error in the retrieved CO values close to the fires?

The difference in observations close to the fires may be due to a lot of different factors: the amount of vegetation burned is one. There is a difference in emissions between the types of vegetations: for example, emission factor for savanna is estimated to 65 g CO / kg dry matter, and to 107 g CO / dry matter for extratropical forests (Andreae and Merlet, 2001).

However, as discussed later in this section, the retrieval over land is very uncertain. Although the retrieval error remains quite small, the RMS error is significantly larger than for retrieval outside the fires over land or in the fire plume over the sea. Clearly, improvements are needed for such retrievals, to improve the retrievals but also to better understand the sources of uncertainty.

Ideally, in situ measurements in fire plumes would be required to quantify uncertainties by providing observations of general atmospheric conditions (temperature, humidity), surface temperature estimates, together with trace gases. For this study, we have chosen to filter out the retrievals with larger RMS on the residuals, including the extreme 22ppm enhancement.

Following the referee's request to add details on the possible effects of saturation, we have undertaken a series of analyses based solely on simulations. The algorithm always tends to underestimate the CO due to smoothing error. These studies have also shown that, in theory, extremely large amounts of CO are needed to induce saturation

of the CO lines. Therefore, this hypothesis doesn't seem to explain the structures of the averaging kernels and residuals observed for IASI.
Several other factors could play a combined role: surface and atmospheric temperatures, possibly water vapor and aerosols (although their impact in the CO band is quite small). Due to the lack of independent in situ observations, we were not able to test these hypotheses so that it is difficult to associate an exact accuracy to the retrievals. In the following paragraphs, we evaluate the level of confidence according to the RMS on the residuals between observed and fitted spectra and apply a filter accordingly in order to remove less accurate retrievals (including the one reaching 22 ppmv).
We have modified the discussion in the paper according to these sensitivity studies and minimize the role of saturation to explain the difficulty of the retrieval.
We have also modified the abstract and conclusion in order to state more clearly that the retrieval of 22ppmv above the fires is more uncertain.

14 – *p.7427 lines 27-29; p.7428 lines 1-2: What is the impact of the different footprint sizes of MOPITT and IASI on the differences presented here?*
The referee is right to mention this issue. MOPITT has a horizontal resolution of 22x22km and IASI of 12km (diameter), so that the average CO concentration measured in each pixel by each instrument can be quite different due to inhomogeneities in the CO distributions. We added a mention to this issue in the text. Larger pixel could result in smaller values if the collocated smaller pixel is right above a large source, or to larger values if the smaller pixel is not above the source. These effects are difficult to account for. Both instruments also have very different retrieval algorithms and different a priori information, so it is difficult to say what effect will dominate. George et al. (2009) compare distributions averaged on 1x1 degree grids in order to avoid this effect and find similar results.

15 – *p.7428 lines 2-3: Is this solely due to the fact that the available MOPITT product only contains cloud-free data, thus rejecting measurements containing smoke, or are other data rejected as well?*

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Both IASI and MOPITT only provide retrievals for cloud-free conditions. It is possible that the cloud detection could mistake large aerosol loadings with cloud-contaminated data. However, the cloud filter for IASI should also remove these pixels. It could also be a specific problem in the processing that filters out some high CO values.

16 – *p.7429 lines 3-11: Why not compare the total CO burden before or after the fire event with the total CO burden during the fire period in order to estimate the CO burden due to the Greek fires?*
The method we chose is close to the one suggested here, except that we chose specific regions for the calculation of the CO burden. This selection of regions was done in order to avoid double counting of the fire emissions (at different stages of transport), and we removed a background that accounts for CO levels around the fire plumes.

17 – *p.7429 lines 8-9: How do tourists cause increased anthropogenic emissions? Because of increased (air) traffic? Or other sources as well? Please explain.*
We removed this statement.

18 – *p.7430 line 19-22: Instead of providing a lot of information between brackets it would be good to write this information out in a few sentences summarizing the characteristics of the MODIS observations used. Please also add the spatial resolution of the MODIS observations used here.*
We have added a rapid description of MODIS in the revised version of the manuscript.

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Anonymous referee 2:

Specific comments on plume height information:

1 –*(p.7414, l.14,24-26; p.7432, l.1-12; p.7435, l.5-7) You find that the measurement contains 1.7 pieces of independent information and then claim that you can distinguish more, namely 3, different plume altitude levels: lower, middle and upper troposphere. This cannot be true and you have not shown any determination of plume height from the IASI CO observations. I suggest removing the conclusion l.24-26 from the abstract. It seems appropriate to mention this issue in an outlook on further studies at the end of the Conclusions section though.*
As suggested, we have removed the last sentence of the abstract.

2 – *(Fig.3,4,12) The averaging kernels (Fig.3,4) exhibit vanishing sensitivity at 1 km altitude, while the retrieved profiles (Fig.12) vary distinctively near this altitude; even the sign of the slope varies. What is the cause of this variation? Since it does not seem to be induced by the observation, I wonder whether it might be induced by the a priori error covariance matrix. Can you discuss the lowest 4 km of the retrieved profile shapes? Anyway, you should explicitly and quantitatively state the a priori error covariance matrix in section 2, as it is a key component of the presented retrieval method.*
A quantitative description of the a priori covariance matrix has been added to the qualitative description of its construction in section 2. Two maxima of variance are visible in the a priori covariance matrix: the main one in the lower troposphere (0-1km layer) reaching 65% (in terms of standard deviation), and the second one in the UTLS (45%). The variability progressively decreases from the lower to the middle troposphere (35%).

This large maximum in the a priori uncertainty near the surface explains that in the most critical cases (larger inversion error close to the emissions), the surface retrieval

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is extremely large and may not be realistic. The profiles with a maximum in the 1-2km layer at the beginning of the fire plume (Fig. 12) have similar averaging kernels as the night-time profile above the sea plotted on Fig. 3, which have large values in the 0-2km layers. Since both retrievals were done in similar conditions, it seems that this maximum at 2 km corresponds to a real feature of the CO profile. This could be due to different fire plumes transported slightly differently (i.e. differences in the fire activity), or to a mixing of the plume during the transport. Clearly, additional analysis would be required to explain this, with for example a regional chemistry-transport model including temporal variability of the fire activity for trace gases and aerosols.

3 – *Fig.12 shows that your retrieval can produce profiles with a pronounced maximum near 2 km altitude. This is exactly what the independent CALIOPE LIDAR observations indicate in Fig.11. Why did the retrieval not reproduce the maximum in this case?*
This question refers to the same issue as the previous one. The maximum at 2km observed in the bulk of the plume on Figure 12 is not observed in the profile compared to the CALIOP lidar observation, and it is not observed everywhere in the IASI fire plume (see Fig. 3). For the comparison with CALIOP, the profiles are not in the larger part of the plume, so the averaging kernels have more sensitivity to the 1st levels (see AK for background above the sea in Fig. 3). The differences could be explained by the difficulty of comparing exactly the same air mass considering the variability in the fire activity. We mention these two questions in the discussion of Fig. 12.

Other specific comments:

1 – *(p.7415, l.23 – p.7416, l.3) Portuguese fires seem irrelevant. I suggest deleting.*
We rewrote the introduction to keep it more efficient and deleted this part.

2 – *(p.7417, l.27 – p.7418, l.2) Please either give a reference or move this statement to the conclusions section.*

We have chosen to remove this statement from the introduction to shorten it.

3 –*(p.7420, l.1) How where the temperature and humidity profiles generated? Are they from the ECMWF operational analysis?* We have added a description of the ECMWF analysis used.

4 –*(p.7423, l.14-15) Cite your source of burnt area estimates.*
Reference to the JRC EFFIS report has been added for burned areas mentioned in the paper.

5 – *(p.7426, l.4-5) You state that the iterative nature of the retrieval approach would act to lower the retrieval error estimate in the retrieval error covariance matrix. According to my understanding of optimal estimation this is not true: According to your Eq. 2, the retrieval error covariance matrix solely depends on the a priori and measurement error covariance matrices and the Jacobian at the retrieved state vector. This is independent of how many iterations were needed to determine the retrieved state vector. Please check and clarify the source of the retrieval error underestimation.*
We agree that the discussion of the retrieval errors is too vague and we have added some details. The retrieval error as provided by the retrieval a posteriori covariance matrix does not include the smoothing error, and, which is more critical, does not include the contribution of the error on other parameters (mainly temperature profile). The latter could be particularly important in the fire plumes but is difficult to quantify due to insufficient in situ observations.
Another important aspect is that the extreme cases observed during fire events are not well represented in the a priori covariance matrix Sa. This means that the retrieval could be biased towards the a priori, and therefore underestimated. This bias is difficult to quantify, and would need to be fully assessed using independent in situ observations.

6 – *(p.7429, l.18-19) Please state the assumed values for specific efficiency and CO emission factor, and cite your source.*

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We have added a short description of our bottom-up calculation of the emissions and give a few relevant numbers.

7 – *This was not shown in the paper. Please provide reference or label as "not shown here".*
We have reformulated this sentence on the impact of inhomogeneities to mention that this is an issue that needs to be further investigated.

Technical comments:
We have decided to keep Eq. 3 since it allows a discussion of the retrieval errors which we think is useful information. All other suggested corrections have been done.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7413, 2009.